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Cutaneous absorption #21

J B Stuart
1810

An Inaugural Essay.

Containing experiments and observations in defence of the doctrine of Cutaneous Absorption.

For the degree of doctor of Medicine.
Submitted to the examination of

The Trustees and Medical Faculty of the University of Pennsylvania.

On the day of

1810 No 57

By

Josephus Bradner Stuart of
Albany, New York

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The subject of Cutaneous absorption having within the last ten years attracted ~~much~~ ~~the~~ attention of some of the most eminent Physicians of this country; and the doctrine of non cutaneous absorption having been very ably supported; many have been induced to adopt it, and some have now supposed it established beyond the reach of controversy.
But after having heard it ably advocated, and after having attentively perused the different papers published relative to it, by graduates in this University; Though I was not persuaded by the eloquence of the former, nor convinced by the arguments and experiments of the latter, yet I was unable at that time to refute them. — — — —
Having been early taught "that one truth in Medicine is worth a thousand unconnected lifeless facts." I could not conscientiously rest my opinion on other mens bare assertions, however great their eminence, either as Physiologists or Physicians. I resolved therefore to avail myself of my first leisure moments to repeat Doctor Murphy's experiments with madder, (the correctness of which had been by some doubted) and at the same time, try such other articles as I might deem most proper; and rest my opinion on the result of those experiments. Accordingly, having engaged my ingenious friend Mr. Thos. P. Jones, to assist me, on the 17th of March 1810 I instituted a course of experiments with the Rubia Tinctorum, Rad. Rhei, Rad. Curcuma, and Garlic. —

Experiment. 1st.

At 15 Minutes past 4 o'clock P. M. having evacuated my urine, I immersed myself (my head and neck excepted) in a strong watery infusion of the Rubia Tinctorum, and remained in it two hours and a half. The temperature of the atmosphere was 34°. that of the bath fluctuating from 82° to 90°

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Urine was drawn at the expiration of 1. 3. 8. 13. 15. 18. 26. and 37. hours after leaving the bath. The first portion was very pale and unusual in quantity, all the other portions (except the last, which was of its natural paleness) were much above the natural color, particularly the 2. 3. 4 & 5. portions which were of a higher color than common Medeira Wine. On adding a solution of the Carbonate of Potash, to the urine drawn immediately before entering the bath, and to that drawn one hour after leaving it, no perceptible change whatever took place, in the color of either of them. Added to the other portions it instantly changed the color of all of them except the last, to a bright Cranberry red; but the portion drawn at the expiration of eight hours gave the brightest color. The last portion or that drawn at the expiration of thirty seven hours was not in the least changed by the addition of the Potash, any further than water or any similar fluid would weaken the color by diluting it. The different portions of urine, which were sensibly changed by the addition of the Potash, on standing 8 hours, deposited a copious white sediment, which was not the case with the other portions. My pulse while in the bath became slower and fuller; and I felt considerable languor and slight head-ache for two or three hours after leaving it.

Experiment. II.

With a view to ascertain whether the change of color produced by the addition of Potash, to the several portions of urine as before mentioned; depended on the presence of the coloring matter of Madder, March 18d. I added to a portion of urine drawn before entering the bath in the preceding experiment, a watery infusion of Madder, until it became

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of the same color as that drawn three hours after leaving it. On adding the Potash to this, it immediately assumed the bright cranberry color. The Potash produced the same change on a weak infusion of Madder in common pump-water.

Experiment III.

March 19th at half past 9. A.M. Mr. Jones having evacuated his urine, immersed himself, his head and neck excepted in a strong watery infusion of the Rad. Rhei and remained in it two hours and a half. The temperature of the atmosphere was 40°. that of the bath fluctuating from 84°. to 95°. Urine was drawn on leaving the bath and at the expiration of 2. 4. 6. 9. 11. 21. 26. and 34 hours afterwards. All the portions except the first and last were very highly colored. On adding a solution of the carbonate of Potash to the Urine before entering the bath, and to that drawn on leaving it, no perceptible chang took place in the color of either of them. Added to the other portions it instantly changed all of them except the last, to a deep red color. On the last portion it produced no sensible chang. All those portions which were reddened, by the addition of the Potash, on standing 20 hours deposited a copious sediment. In those drawn at the expiration of 9 & 11 hours it was more copious and of a pale red color. While in the bath, his pulse was increased in force, but not much if any in frequency. No languor or headache succeeded. in

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Experiment IV.

In order to ascertain whether the color of the Urine as last mentioned depended on the presence of the coloring matter of the Rhei. I made a watery infusion of Rhei of a similar color to that of the Urine, drawn four hours after leaving the bath. On adding the Potash it instantly assumed the same deep red color of the urine above mentioned.

Experiment V.

March 20th at 15 minutes before 3 P.M. I immersed myself (my head and neck excepted) in a strong watery infusion of the Rad. Curcuma. and remained in it two hours and a half. The temperature of the atmosphere was 45°. that of the bath fluctuating from 86° to 95° degrees. Urine was drawn on leaving the bath, and at the expiration of 2. 5. 12. 16. 21. 28. & 34 hours afterwards. All these portions except the first and last were much above the natural color. & on adding a solution of caustic potash they instantly assumed a reddish hue, — tho. in a much less degree than either of the preceding articles. Those drawn at the expiration of 2. & 5 hours gave the high color. That drawn 12 hours after leaving the bath, on standing 15 hours, deposited a copious sediment of a dusky brown color. On adding the Caustic Potash to the Urine drawn on leaving the bath. and at the expiration of 34 hs

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Experiment VI.

In order to ascertain whether the color of the Urine could be altered and also to determine whether the Caustic Potash was a proper test to detect the presence of Curcuma, I made a watery infusion of Curcuma similar in appearance to the highest colored urine in experiment 5. On adding the Caustic Potash to this and also to the Urine above mentioned they both assumed precisely the same red color.

Wishing to ascertain whether the odor of certain volatile substances could be taken into the system in a manner similar to the coloring matter of the preceding articles, I made the following experiment with Garlic. Taking the state of the Urine and breath as a proper criterion.

Experiment VII.

March 21st at 4 P.M. I took one end of a tube into my mouth, the other end of it was placed out of a window, several adhesive plasters were then applied over my mouth and nose so as to completely prevent the passage of air, the tube was then held, except when taken the

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I then applied cataplasms of bruised Garlic to my axillae, to the inside of my thighs, and upon the operation, one hour and a half, as they produced considerable pain, they were removed, and the parts washed with warm soap suds, to which they had been applied. I then left the room immediately, and a few minutes after again washed the parts with soap and water, and changed my clothes. After which I took a walk of a mile. One hour and a quarter after the Garlic was removed, my breath was sensibly tainted with the odor of Garlic so much, so that two gentlemen that were in company with me at the time mentioned it to me. Two hours after the time I first noticed a change in my breath, that it was not only very disagreeable to myself, but very perceptible to several persons that I was in company with, and it continued to be so until bed time. On rising from bed next morning 14 hours after making the experiment, nothing of the odor of Garlic could be perceived in my breath. The urine was frequently examined during the thirty hours succeeding the experiment. The perspiration during the first two hours after removing the Garlic, exhibited nothing peculiar with

odor or odor. At the expiration of six hours it had a disagreeable pungent smell, and at the expiration of fourteen hours it was still more so. But the smell of Garlic could not be perceived in it. This disagreeable pungent smell continued for twenty six hours, after which it became imperceptible. —

Experiment VIII.

With a view to ascertain whether ~~whether~~ Garlic when taken into the stomach communicates any odor to the urine, I took 3j of Mr. Jones at several doses of Garlic. his urine was frequently examined during the succeeding 36 hours. At the expiration of two hours after eating the Garlic nothing peculiar could be perceived in the odor of his urine But at the expiration of 4. 6. 8. 12. & 24 hours. It had precisely the same disagreeable pungent odor of the urine mentioned in experiment VII. At the expiration of 36 hours it had entirely disappeared. The result of this experiment, which fully establishes two points in question 1st That Garlic when taken into the system does communicate a peculiar odor to the urine, but it is one essentially different from that of the Garlic — — — —

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[illegible] that the [illegible] odor of certain articles of the [illegible] mentioned in experiment VII. was produced by some [illegible] of the Garlic used in the experiment. [illegible]

Having concluded the preceding experiments, I could not for a moment hesitate ~~on~~ which side of the question to advocate. For the articles used, or at least some part of them, appear to have actually entered the system; and can any one for a moment suppose that it is possible for the three first articles, which it is known are not volatile, to have been taken in by the lungs? Through what other medium then could they pass, if not by that of the pulse? As it respects the experiment with the Garlic, considering the complete manner in which the lungs were excluded from any agency in the business, I think the result fully as conclusive as either of the preceding. The very result itself proves clearly to my mind that the odor was not taken into the system by inspiration. For if it had been so, not my breath have been more strongly impregnated with it at the conclusion of the experiment

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[illegible] a [illegible] hours, would it not have gradually decreased? If then not only the coloring matter and [illegible] would not be more [illegible] certain odors but the volatile odor of others, when applied to the surface of the human body is conveyed to the system by means of cutaneous absorption, may we not reasonably suppose that Mercury is conveyed into the system in a similar manner when applied to the surface of the body in the form of Unguentum Citrini?
If this is not the case, how, I would ask is, a salivation produced by its use in this way? I have seen some of the accounts for non cutaneous absorption, say in answer to this "that the Mercury was volatilized and [illegible] into the lungs or that it was induced by a sympathy with the glands of the mouth" are those to which the Mercury has been applied or probably in both ways. In the instances we have to observe, that it requires a degree of heat far above that of the human body, to volatilize Mercury. Therefore till they bring some proof of its being so, I have very reason to doubting it. ——

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But admitting it to be a fact. I would ask, how it happens that the attendants in the Venereal Wards of Hospitals, (who would in such a case, be constantly in a mercurial atmosphere;) are not frequently salivated? —
Such instances have perhaps seldom, if ever occurred.
To the second, I would only observe, that of late it has been so fashionable to refer every thing to sympathy which cannot be readily accounted for some other way; that it would have been heresy in any one to doubt it or to have over looked it on the present occasion.
By some it has been said "Whatever the matter which is the cause of most of the diseases (particularly those which are strictly febrile) which afflict mankind, floats in the atmosphere, it is not reasonable to suppose that the surface of the human body, is endowed with the power of absorbing. For if this be the case, it would be hardly possible, for persons who expose themselves to the open air (particularly in sickly seasons) to escape disease." To me this appears to be a very futile objection. For if we were to determine the question by reasoning in this way, it would be much more plausible, to deprive the lungs of the power of absorbing. (A power that no one at this day denies) —

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and confine it exclusively to the skin; in which case we could in a great measure guard the system by means of proper cloathing. Whereas we are always under the necessity of breathing the circumambient air.- In making the preceding experiments, every attention was paid to have them done accurately. For having espoused neither side of the question; I felt no farther interest than in the result than truth might be concerned. If the experiments have been correctly and properly made, I think the doctrine of cutaneous absorption must (at least to a certain degree) be admitted. And I think I am warranted in concluding, that certain substances (probably all of those which are either nutricient, or medicinal) do, when applied to the surface of the human body, pass into the system by means of cutaneous absorption.

But should it hereafter appear, that there has been a fallacy, (which if there has I protest I am ignorant of) in the foregoing experiments; and that the substances used passed into the system through some other medium, than that of the cuticle. I pledge myself to be one of the first to renounce the doctrine which I have now advocated.